MODERN TELEGRAPHY.

SOME ERRORS

OF DATES OF EVENTS AND OF STATEMENT

IN THE

HISTORY OF TELEGRAPHY EXPOSED AND RECTIFIED,

BY

SAMUEL F. B. MORSE, LL.D.,

INVENTOR OF THE

RECORDING OR GENERIC TELEGRAPH,

&c., &c., &c.

Τηλε γραφω:

I WRITE, AT A DISTANCE.

INTRODUCTION.

From the title *Modern Telegraphy*, which I have given to this work, it must not be inferred that I am adding another history to the many full and valuable treatises on the telegraph which abound in the book marts of every country.

The second part of the title more exactly defines my principal purpose. The histories already given to the world are so ample on almost every branch of science and art bearing upon the telegraph that any thing I could add would, for the most part, be but a repetition of information already better presented by their acute and learned authors.

While perusing their writings, however, with the deepest interest, I could not be but painfully impressed with many misstatements and errors, personally affecting me, which disfigure their narrative and mar its accuracy, for although the general and well pronounced verdict of the world has apparently not been greatly influenced by these errors, yet I may be excused if, in regard to some of them, affecting personal character, I should manifest some sensitiveness and think that *matter of fact* is better than *misstatement*, especially since I possess the means of disabusing the minds of those who have been honestly mistaken.

Errors of *date* are corrected by a more detailed account of the incipient steps in my invention of the telegraph, and by the collation in an *Appendix* of evidence, mostly taken from the mass of testimony in the voluminous records of the Courts, evidence which is pertinent to prove the accuracy of the true dates. One case of peculiar flagrancy, the preposterous pretensions of C. T. Jackson, a geologist of Boston, which have been made by some writers the means of disparagement to my claims to originality in my invention, I have deemed it proper more thoroughly to expose, since the question involves *cha-*

racter for veracity, *mine*, as well as *his*, the oaths of each in Court being in direct opposition. Other cases of injustice towards me, in which the remarks of the historian have been evidently biased by assuming the truth of Jackson's representations, may also require individual investigation and exposure at a future day. If any exposure be deemed necessary, each case will be made the subject of a distinct paper.

With these brief remarks I submit what I have written to the candid judgment of the impartial reader.

Paris, June, 1867.

MODERN TELEGRAPHY.

The project of *Speedy communicating by signals to and from a distance* is not of modern date, nor are many of the modes of the present day by which this great desideratum has been accomplished altogether new. To all modes, however, has been indiscriminately applied the generic name *Telegraph*.

It may not be amiss, therefore, in the outset, to impress upon the mind more definitely than has yet been done the literal meaning of the word TELEGRAPH, since it has so generally been adopted to designate, *all modes of communicating at a distance.*

From the etymology of the word, τηλε *afar off*, *to a distance*, and γραφω, to *write*, *imprint*, or *describe*, literally signifying *to write afar off*, it is at once perceived that until a very late period all the methods of communicating at a distance, employed from the earliest ages, do not correspond strictly to the name *Telegraph*.

The word *Semaphore* from its etymology σημα *a sign*, or *signal*, and φερω *to bear* or *convey*, much more accurately defines the character of all the ancient and many of the modern modes of communicating which go by the general name of Telegraphs. This nature of the word, therefore, it is necessary to keep in mind, if we would clearly understand the character of the improvements that in this age have been made in the means of communicating intelligence to a distance.*

If we examine these various means, of whatever kind, previous to the year 1832, it will be found that, without exception, they are by *evanescent signs* or *signals.*

Assuming then as a suitable stand point the year 1832, and taking a retrospective view from that date, it will be found that the annals of history, previous to 1832, will be searched in vain for a single hint of a method of communicating at

* In my definition and employment of the word *Telegraph*, I wish to guard against misapprehension, and a charge of inconsistency, since I have myself formerly used the word in the general and erroneous sense which I condemn and propose to correct. It is in the general sense only, as applied to *all modes of communicating at a distance*, that I disclaim to be the inventor of the *generic telegraph*. I do claim to be the inventor of the *first real telegraph*, and consequently of the *generic* telegraph, not of the *first semaphore*.

a distance, which literally fulfils the definite result inherent in the word Telegraph, or even for a suggestion of the possibility of accomplishing such a result, except so far as the word itself in its etymology foreshadows it. It is clearly metonymic.

In verification of this assertion, I quote a passage from my *American Letters Patent* of the 11th of April, 1846, reissued on the 13th June 1848. In the Preamble I say:

"Various modes of telegraphing* or making signs or signals at a distance have for ages been in use. The signs employed heretofore, have one quality in common. They are *evanescent*, shown or heard for a moment, and leaving no trace of their having existed. The various modes of these *evanescent* signs, or signals, have been by *beacon-fires* of different characters" [by *lantherns*], "by *flags*; by *balls;* by *report of fire arms;* by *bells* heard from a distant position, by *moveable arms* on posts, etc.

. . . . . .

"The original and final object of my telegraph is to *imprint characters*, at any distance, as signals for intelligence; its object is to *mark* or *impress them* in a *permanent* manner."

If then the philological position be taken (and who can oppose a valid objection to it?) the true inventor of the telegraph proper is he who first produces a result in the strictest conformity with the literal signification of the word. The word, indeed, foretokens a future *perfect* result, but it has hitherto been metonymically applied to the various imperfect approximations to it.

Lest any one should charge that this strict construction of the word Telegraph is unusual or forced, permit me to quote from the opinion of Justice Grier of the Supreme Court of the United States, delivered in the Telegraph case in Washington in 1854. In describing the nature of the invention the learned Judge says: "It is not a composition of matter, or a manufacture, or a machine. It is the application of a known element or power of nature to a *new* and useful purpose by means of various processes, instruments, and devices, and if patentable at all, it must come within the category of "*A new and useful Art.*" It is as much entitled to this denomination as the original *Art of Printing* itself.

"The word Telegraph is derived from the Greek, and signifies to WRITE *afar off or at distance*. It has heretofore been applied to various contrivances or devices to communicate intelligence by means of signals or semaphores which speak to

* The word *telegraphing* (it will be observed), in the Letters Patent at that date, is used in its usual loose sense, and not in the rigidly strict sense that I have insisted upon in my remarks. Yet the word even here is followed by the definition "making signs or signals at a distance," which confines its application to the *semaphore*.

the eye for a moment; but, *in its primary and literal signification of* WRITING, PRINTING, *or* RECORDING *at a distance*, it never was invented, perfected, or put into practical operation till it was done by Morse."

The position I have taken, therefore, has the strongest support from one of the highest legal authorities, and one of the most discriminating and clear-headed jurists of the United States, whose opinion was concurred in by others, his associates in the Supreme Court. If this position be conceded my invention must be termed the *Generic Telegraph*. This definition is certainly more accurate, and furnishes the means of a more logical classification, and, therefore, makes clear much that is confused in the history of the Telegraph. But even if objected to as an innovation, as an unusual limitation of the meaning of the word, as against common usage, which has sanctioned its use as expressing all modes of communicating at a distance, still the important fact of the birth of a new *mode* of communicating at a distance is not obliterated by disallowing the strict definition of the name.

Whether my invention, in accordance with the strict meaning of the word, is called the *first real Telegraph*, or only a *new system of Telegraphing*, the distinguishing feature of it is still the same, by whatever name it may be called; an entirely *new mode* of Telegraphing was at least invented, and not an *improvement* on an *old mode*. The needle system in 1836 was an old semaphore mode, and Messrs. Cooke and Wheatstone and others modified and improved it; but in 1832 a mode altogether new was invented. *Automatic recording* as applied to communicating to a distance was a new mode, not even conceived possible before 1832. Improvements or modifications in recording have since been made, but my claim has ever been to the *original Recording Telegraph*.

Modern science has made most opportune additions to the means of rapid communication of thought in furnishing the discoveries in Electricity, especially in the more recently modified forms of voltaic or galvanic electricity, and magneto-electricity for adaptation to this purpose; yet it must be conceded that previous to the date 1832, which we have assumed as a stand point, electricity was also exclusively applied *semaphorically*, and in no instance *telegraphically*.

Electricity is, indeed, the only agent, or at least the best known agent, peculiarly adapted to a true *telegraph*. It is applicable, of course, and is, therefore, applied to *semaphores*, but while many other agencies are adapted to semaphores, *electricity* alone is adapted to *Telegraphs*.

These two titles SEMAPHORIC and TELEGRAPHIC very distinctly divide and designate the two classes of inventions, having for their common end *the communicating of intelligence to a distance.*

The *semaphore* holds exclusive possession of the period previous to the year 1832, and it is not until this date that the *Telegraph* proper appears upon the stage.

In this view of the question, who was its inventor?

In examining and settling the dates and character of inventions, especially of one of such world-wide interest, it does not suffice to confine one's researches to the narrow limits of a circumscribed locality, or to a single country. The whole civilized world is the field for examination.

America, if separated from Europe by a wide ocean, is yet not so far distant as to prevent free interchange of ideas. Nor does it now so belong to that section of the unexplored world denominated in the old maps, *terra incognita*, that either its material or intellectual property, the product of its own region, may be appropriated without ceremony by any who choose to take it on the plea of the right of discovery, that ill-defined and very doubtful right enforced in a barbarous age. It is scarcely necessary more than to hint that American inventions have had some influence in modifying for good the affairs of the rest of the world, and, therefore, she may claim to be fairly considered in an investigation of questions respecting the invention of the telegraph, especially since one of the prominent claimants is an American citizen.

I humbly trust, however, that in endeavoring to establish my own claim, to be the first inventor of a *recording or printing telegraph*, I shall not justly be accused of egoism, or of a wish, or intention to derogate from the credit for originality or merit of any whose labors are illustrious in the same wide field of invention.

I have had so many striking and flattering proofs of the prevalent verdict both at home and abroad which regards me as the original inventor of the system of telegraphs which bears my name, and which is now universally adopted throughout the world, that I have felt some reluctance to treat of the Invention, lest my motives be mistaken, conscious that the abundant evidence contained in the archives of the United States Courts, and in public documents and letters, is amply sufficient to satisfy the candid inquiries of any future historian of the telegraph. Still it has been urged upon me by many friends that I could spare an historian much labor if, with the facts fresh in my own memory, I would commit them to writing. A request urged from such motives I could not resist.

I am pursuaded, however, that my claim may be so asserted as not justly to give umbrage to any fair-minded person.

It is asserted by some writers that the controversy respecting the inventor of the telegraph is "still unsettled," and so "constantly renewed,"

Why should this be? May it not be owing in some degree to the unsatisfactory modes, adopted in most instances to prove priority? Note for example the chronological table mode,* in which arbitrarily selected dates of certain events in the progress of various inventions are arranged in chronological order, events otherwise interesting but irrelevant in settling the main question, and manifestly unsatisfactory, because the selections and omissions are easily so disposed as to make a false impression.

For this purpose, it is only necessary, to select dates pertaining to that invention, which it is desired to make prominent, and omit altogether any notice of other inventions. Although every part of a table so constructed may be strictly *true*, it may, as a *whole* by its simple omissions, be actually *false*.

The chronological mode may conveniently and properly serve to mark the successive steps or stages in the progress made by the inventor in his bringing forth and completing his individual invention, but it is fallacious when applied to the collective events of *different* inventors treated as one invention.

As a consequence of this mode of history it is often assumed that an invention is prompted and perfected by a knowledge of former attempts by others to realize a similar result. The dates of these are recorded in chronological sequence, as if the latter attempts depended upon and were the resultants of the former. Such sequence is often imaginary. Indeed it seldom exists. It is much more frequently the case that an invention is conceived and perfected wholly in ignorance of any previous attempts by others. However plausible, therefore, such a classification may seem as a means of settling priority of invention, it in reality settles nothing.

Again, in settling the question of priority of invention between two or more claimants, an important *preliminary question* seems to be wholly left out of view. Are the inventions, the priority of which is contested, *alike*? The necessity for first deciding this question is obvious, for it is only on the supposition that *they are alike* that the question of priority becomes significant. If they are *alike* then, indeed, the evidence must determine to which of two or more claimants for *one and the same thing* the invention belongs. But if they are *not alike*, if they are different inventions, although loosely designated by the same generic name, each claimant may have a just title to be considered the contriver of his own invention, and the question of priority among the different inventions is reduced to one of simple chronological position without derogating aught

* See Highton [page 38 and 39] for example.

of credit or originality from either inventor. The question for example whether the steam engine, or the spinning-jenny were first invented surely may be settled without disparagement to the claims to originality of either of the inventors of these useful inventions.

Discoveries and inventions, and experiments made in one part of the world, even if published in standard scientific journals, are not therefore as a matter of course, immediately known in other countries. Many, if not most, of them attract little more than a temporary or local attention; some it is true are widely circulated, but it is more often the case that the most important when first announced are perused with doubt, receive a transient notice, and are then immured in the alcoves of a library, and pass into comparative oblivion, or are remembered only as *failures*. They reappear, galvanized into life, when the success of some later inventor, wholly unapprized of these previous attempts, triumphantly accomplishes the result that had baffled the efforts of others. They are then disentombed and marshalled in chronological array to derogate from the honors which the world is ready to accord to the successful inventor, or sometimes for the more unworthy purpose of pirating from him his pecuniary reward.

Dr. W. H. Russell, the distinguished clear-minded correspondent of the London *Times*, in his most interesting work on the Atlantic Telegraph, lays down an indisputable common sense rule for ascertaining the true inventor in cases of disputed claim, one which at once commends itself as eminently just. He says:

"He who first produces a practical result—something which, however imperfect, gives a result to be seen or felt, and appreciated by the senses—is the true ποὶητης the maker, and inventor, whom the world should recognize, no matter how much may be done by others to improve his work, each of these improvers being, after his kind, deserving of recognition for what he does."

I see no objection to this just and discriminating rule, which apportions to the various laborers in the same field, from the chief originator to the humblest improver, his appropriate honor and reward.

If it be asked why I have assumed the date of the year 1832 as a stand point? I reply: because at that date the idea was first conceived, and the process and means first developed, which has completely revolutionized the means of communicating at a distance throughout the entire world. If not even the *idea* of the possibility of recording or imprinting at a distance was originated previous to that date, there is an evident propriety in making prominent the date of its birth. If in the year 1832, not only the idea was originated but the *process and*

means were devised for successfully carrying into effect the novel thought, it becomes still more an epoch worthy of special note, particularly if it can be shown that the process and means then devised are the same process and means substantially in universal use at the present day, not in a circumscribed locality nor in a single country, but literally throughout the world.

In what condition then was invention in the modes of communication to a distance in the year 1832?

In 1838, being in Paris for the purpose of making known my Telegraphic invention to the European world, through the Academy of Sciences, I commenced, in the library of the Institute, the research for any previous attempts to use electricity for purposes of communicating at a distance; and I succeeded in extracting from the archives the following notices. Lomond's attempt of 1787; Reiser's, 1794; Salva's, 1798 (?1796); Soëmmerring's, 1809 (?1811); Schilling's, 1833; Steinheil's, 1837; Masson's, 1837; Amyot's, 1838, intending myself to write the history of Electric Telegraphs. Finding, however, on my return to the United States, my whole time necessarily occupied in establishing my own system, I laid aside these notices for a future day. But my colleague and friend, the late Alfred Vail, Esq., being desirous of writing such a work, I handed over to him these notices, which he used in his work, *The American Electro-Magnetic Telegraph*, published in August, 1845. To Mr. Vail is due, therefore, the credit of writing the *first* history of Electric Telegraphs; however imperfect, it was of necessity. But to Monsieur l'Abbé Moigno the credit belongs of a more elaborate and complete work on the subject, highly interesting and valuable, in spite of its defects, and the unworthy prejudices and favoritism which disfigure it. The learned and enthusiastic Abbé commences his Treatise on the Electric Telegraph with a rapid glance at the first means of communicating at a distance, dwelling more at length on the mechanical semaphore of the ingenious Messrs. Chappes, the best of all this class previous to the application of electricity to that purpose. He then passes over the ground already traversed by Mr. Vail, adding, however, Ampère's* important suggestion of 1820 (the first suggestion of the needle semaphores), until he reaches the year 1832, at which date he indeed introduces my name and invention, but with so much manifest scepticism, with so much disparagement, and with such apparent unwillingness to allow either date or invention the weight due to them, that he expends two or three pages in qualifying and discrediting, and depreciating, both the evidence and the

* It seems to be overlooked that Ampère himself gives the credit of the suggestion to La Place. "Cette expérience m'a été indiquée par le savant illustre La Place."—*Annales de Physique et de Chimie*, t. XV., p. 72.

inventor. As I may have occasion in another place to comment upon blemishes, in the otherwise valuable work of the Abbé's, I now pass on to remark that the Abbé's chronological narrative of all the previous devices for communicating at a distance confirms the statement I have made, that up to the year 1832 none of the contrivances for communicating at a distance, either by electricity or by any other means, contemplated an *automatic record*. If then, in 1832, not merely the novel idea, but the means and process were also devised of a method of communicating at a distance, which has been so successfully embodied as to supersede almost entirely all other methods throughout the world, does not such a fact deserve a more prominent position, and a more gracious reception, than has been accorded to it, either by the Abbé or by many other historians of the telegraph?

If this rapid retrospect, previous to 1832, substantiates the fact that all modes of communicating at a distance were without exception *semaphoric*, and that no strictly *telegraphic* mode then existed, what occurred to change this condition of things in 1832?

In the month of October, 1832, I left France for the United States in the packet ship *Sully*. Early in the voyage, in the course of conversations in the cabin, some of the facts in relation to electricity, familiar to me from my college days,* were casually brought to my recollection in describing the then recent discovery of the means of obtaining the electric spark from the magnet; a discovery which demonstrated the intimate relations of magnetism and electricity. The fact that electricity passed with such rapidity through a space of many miles was alluded to, in which Franklin's opinion of the *tantaneity* of the passage of electricity was brought to notice. his led me to remark, that "if that were so, and the presence of electricity could be made visible in any desired part of the circuit, I see no reason why intelligence might not be transmitted instantaneously by electricity." This was the crude seed which at once took root, and with the favoring leisure of a long voyage, and a mind unoccupied with other studies, grew into form, absorbing my thoughts in the sleepless hours of the night, and turning the tedium of the voyage into an agreeable pastime. Before the end of the voyage the invention had the following attributes.

I may observe in passing that my aim at the outset was simplicity of means as well as result. Hence, I devised a *single circuit of conductors* from some generator of electricity. I planned *a system of signs* consisting of dots or points, and

* See Appendix, documents XV., XVI., Professor Silliman's and President Day's depositions.

spaces to represent numerals; and *two modes* of causing the electricity *to mark or imprint* these signs upon *a strip or ribbon of paper*; one was by *chemical decomposition of a salt* which should discolor the paper; the other was by *the mechanical action of the electro-magnet*, operating upon the paper by a *lever* charged at one extremity with a *pen or pencil*. I conceived the plan of moving the paper ribbon at a *regular rate* by means of *clock-work machinery* to receive the signs. These processes, as well as the mathematically calculated *signs*, devised for, and adapted to *recording*, were sketched in my sketch-book. I also drew in my sketch-book modes of *interring the conductors in tubes in the earth*, and, soon after landing, planned and drew out the *method upon posts*. This was the general condition of the invention (with the exception of the plan upon posts), when I arrived in New York, on the 15th November, 1832.

Among the original characteristics of the invention as devised on board the ship, one of the most important was the mathematically calculated *signs* adapted to *recording*. As these signs have ever since played a most important part in the modern telegraphs, they would seem to demand here a more distinct notice of their origin.

In reflecting on the operations of electricity as a proposed agent in telegraphy, I was aware that its presence in a conductor of *moderate length* could be indicated in several ways. The physical effects in a shock; the visible spark; visible bubbles during decomposition, and marks left from decomposition; its magnetic effects upon soft iron and steel; and its calorific effects; these were all well known phenomena. Could any of these be made available for *recording*, and at a *great* distance? This was the important problem to be solved. Electricity had been flashed many miles through a conductor, apparently instantaneously, and produced some of these effects at a distance; may not all of them, likewise, be produced at a distance? If so, which of them seemed to promise the surest result of a *permanent record?* Static electricity, as an agent, was first proposed, but was quickly dismissed as too uncontrolable, and I directed my attention exclusively to the phenomena of Dynamic electricity. The decomposition of a salt having a metallic basis would leave a mark upon paper or cloth, but what salt? Some would probably answer the purpose. Assuming, therefore, that such a salt could be found, how was it to be used? If a strip of paper or cloth were moistened with the salt and was then simply *put in contact* with a conductor charged with electricity, would there be any effect upon the paper? A magnetic effect is produced exterior to the charged conductor; is there any salt or substance so sensitive as to be affected either by decomposition, or in any

other way, by this magnetic influence, by *simple contact* with an electrically charged wire? It was doubtful, but worth an experiment.

But, if such effect were verified by experiment, it was conceived that marks like those in the diagram [1] might be

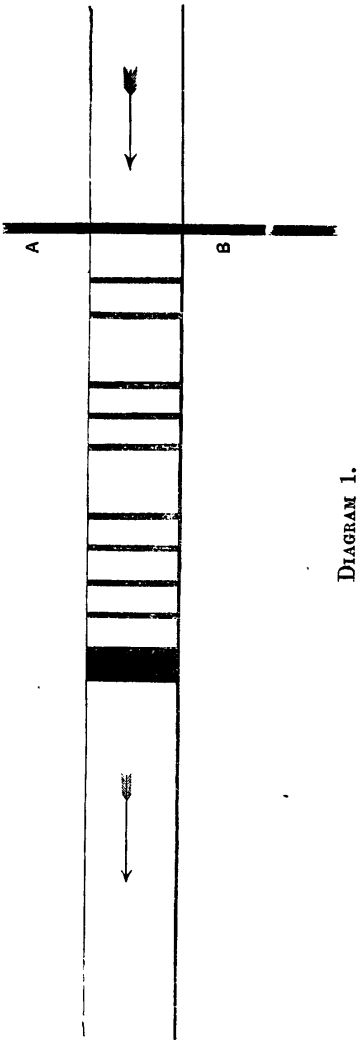


DIAGRAM 1.

made across the moistened paper, as it passed beneath and in contact with the conjuctive wire A B, when the wire was electrically charged and discharged.

It is needless to add that on trial no such effect was produced by the *magnetic* properties of an electrically charged wire upon any salt that I afterwards submitted to the experiment. Nevertheless it is perceived that had this device (which was noted down for testing) been verified, the simplest of all modes of *recording* would have been the result.

The nearest approach to this simplicity seemed to be the passing of the chemically prepared paper between the two broken parts of a circuit so that the electricity should pass through the moistened paper or cloth; this would mark a point or dot when the circuit was closed, and by rapid closing and opening of the circuit, while the paper was moved regularly forward, points or dots, in any required groups, could be made at will. But what salt would best produce this result was to be determined after reaching the end of the voyage. In the mean time, as I originally proposed to record numerals only, intending to indicate *words* and *sentences* by numbers, it was a desideratum to arrange the ten digits to be represented by dots or points within as small a space as possible. The first and most obvious mode seemed to be the following:

1	2	3	4	5	6	7	8	9	0
.	..	...	....	.....	.....	.....	.....	.....	.....

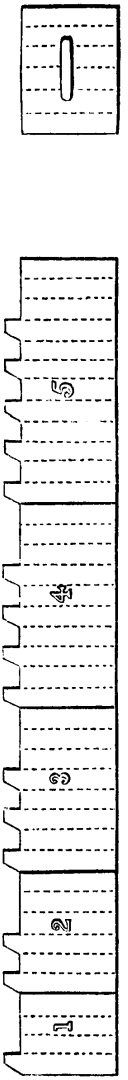
but a few minutes reflection showed that after *five* dots or points the number of dots became inconveniently numerous in indicating the larger digits; hence it occurred to me that, by extending the spaces appropriated to the five larger digits, giving them a greater space value than was possessed by the five smaller digits, I might reduce the number of dots, necessary to indicate any of the ten digits, within five dots. On this principle, therefore, I constructed the following *signs* for the ten numerals, and devised the TYPES for regulating the opening and closing of an electric circuit. [See diagram **2.**]

On inspecting the diagram [**2**] it will be perceived that the types were to be divided into definite *parts*.

Type	1	contains	4	parts, and	appropriates	1	part to its cog, and	3	to its space.
„	2	„	6	„		3	parts to its cog, and	3	„
„	3	„	8	„		5	parts to its cog, and	3	„
„	4	„	10	„		7	parts to its cog, and	3	„
„	5	„	12	„		9	parts to its cog, and	3	„
„	6	„	6	„		1	part to its cog, and	5	„
„	7	„	8	„		3	parts to its cog, and	5	„
„	8	„	10	„		5	parts to its cog, and	5	„
„	9	„	12	„		7	parts to its cog, and	5	„
„	0	„	14	„		9	parts to its cog, and	5	„

Each of the *first* five digits, therefore, are indicated by a space of three parts, and

Each of the *last* five digits are indicated by a space of five parts.



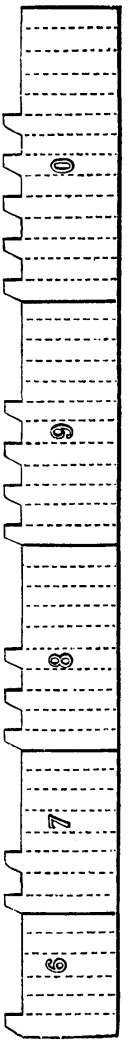


DIAGRAM 2.

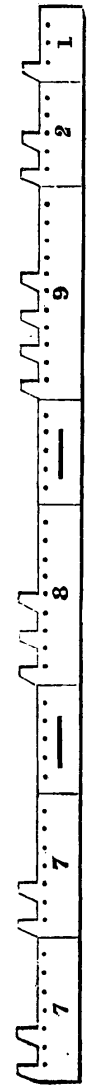


DIAGRAM 3.

The *space type* for separating completed numbers, whether single or compounded, contains six parts.

The *length* of the *spaces*, therefore, was an element to be used in determining the difference between the class of the first five digits and the class of the last five digits, and not simply the number of dots or points. Whether one dot was to be read as numeral 1 or as numeral 6 was to be determined by the length of the space after it, and for the purpose of measuring this space in the last numeral of a despatch the single dot or point was to be used as a supernumerary *finale* to every despatch.

A space of the length of nine or more parts, after a dot or group of dots, indicates the dot or group of dots to be a complete number, whether single or compounded.

A space *less* than the length of nine parts, after a dot or group of dots, indicates that they are a portion of a compounded number.

An example will illustrate this first mode of recording that was proposed. Suppose the numbers to be telegraphed are

77—8—92

The type would be arranged as in the above diagram [3]. The record would show two dots, then a space of five parts, which, being less than nine parts, determines the two dots to belong to numeral 7, the five parts being its proper or natural space, and that it is one of a compounded number, then follow two similar dots, but followed by a space of eleven parts, which, consisting of more than nine parts, shows that a space type of six parts has been inserted, separating this last group from the next; six parts subtracted from the eleven parts leave five parts for the proper or natural space of the last numeral, showing it to be like the first, the numeral 7. Next come three dots, and also followed by a space of eleven parts, which, consisting of more than nine parts, isolates the numeral and shows that a space type of six parts must be subtracted from the eleven, leaving five parts for the natural or proper space of the last numeral, indicating, therefore, the numeral 8. Then come four dots, followed by a space of five parts, which, being less than nine parts, shows the four dots to be a numeral belonging to a compounded number, and that it belongs to the class of the five larger digits, and indicates the numeral 9. Next come two dots, followed by a space of three parts, which, being less than nine parts, shows it to belong to the class of the first five digits, and therefore indicates the numeral 2, because it is succeeded by the final 1, which is not to be regarded except as serving to measure the space to determine the character of the previous numeral.

This method (in the light of my improvements of the code, which very soon followed after the first practical test) seems

crude and even impractical, especially in view of my perfected Alphabetic code devised as early as 1835, and now with some comparatively slight improvements in use throughout the world. But cumbrous and inconvenient as it was, in its earliest stages, if compared in its results with the results of the Semaphoric modes in use at that day, it will be perceived that it was even then a great step in advance.

A day had scarcely passed after my landing, before I commenced the construction of the invention from the plans and drawings made on board the ship. The signs to be recorded or imprinted it was necessary to embody in a species of *type*, the name I gave to the cogged pieces which were to make the required closings and openings of the circuit of conductors, necessary to mark or imprint the points or signs for numerals, upon the strip of paper, at the regulated intervals of time. The paper or ribbon having a *regular* movement, while the type performed the closing and opening of the circuit at *irregular* intervals and thus broke the continuous line of the regular movement of the paper into *irregular parts* at pleasure) and furnished the means of breaking the line into dots and spaces, in such variety, as at once to enable me not only to construct the numerals, but eventually as will be seen by the different combinations of long lines, short lines, or points, and spaces, all the different letters of the alphabet. The *type* proposed at this time consisted of thin strips of type metal with cogs varied at intervals as seen in diagram [2]. These by means of a mechanical movement (hereinafter described) were made and intended for closing and opening the circuit at the desired times. These type, therefore, for imprinting at a distance, were, at that time, an essential part of the machinery in process of construction, and having more facilities, immediately on my arrival, for elaborating these types than for other parts of the machinery, they were the first constructed. A mould of brass was made and a quantity of the type was cast before the close of the year 1832. [Appendix, Doc. II, III, IV, V.] The rest of the machinery, except a single cup battery, and a few yards of wire, and the train of wheels of a wooden clock, which I adapted to the service of unrolling the strip of paper, I was compelled, from the necessities of my profession, to leave in the condition of drawings until I found some more permanent resting place. From November 1832 until the summer of 1835 (two and a half years) I had changed my residence three times, and was wholly without the pecuniary means for putting together and embodying the various parts of my invention in one whole. But in July 1835 I took possession of my new home, in the new building of the New York City University, and I then lost not a day in collecting the parts and putting into practical form the first rude instrument

which was to demonstrate the operation of the invention. I was favored with a little leisure from the unfinished condition of the University building, which impeded the access of visitors to my apartments for my usual professional duties.

I ought here to say that, with the aid of a single cup battery as early as 1834 previous to my removal to the University I ascertained that no visible effect was produced upon numerous salts, which I submitted to trial by putting them in *simple contact* with a wire charged with electricity, as shown in the plan of diagram [1], proposed for experiment on board the ship. I succeeded, however, in marking by chemical decomposition, when the electricity was passed *through* the moistened paper or cloth in 1836, in the University, but the process was attended with so many inconveniences that it was laid aside for the moment, not *abandoned*, that I might give my attention more directly to the *Electro-magnetic mode* of Recording.

If my nomadic mode of life for two years previous, and the condition of my pecuniary means be kept in mind; if, also, it be considered that many of the mechanical facilities in New York, so abundant at the present day, for embodying the invention, did not exist, and, therefore, were denied to me, it will account both for the slowness in completing the instrumentalities of my invention, and the rudeness of the first constructed instrument. The *electro-magnet* was not an instrument found for sale in the shops, as at this day; insulated wire was no where to be obtained, except in the smallest quantities, as bonnet wire of *iron* wound with cotton thread. Copper wire was not in use for that purpose, and was sold in the shops by the pound or yard at high prices and also in very limited quantities.

To form my electro-magnet, I was under the necessity of procuring from the blacksmith a small rod of iron bent in horse-shoe form; of purchasing a few yards of copper wire, and of winding upon it, by hand, its cotton thread insulation, before I could construct the rude helices of the magnet. I had already purchased a cheap wooden clock and adapted the train of wheels to the rate of movement required for the ribbon of paper.

I needed a proper support for the machinery on which to arrange the various disconnected parts. A stretching frame for canvas, XX, diagram [4]* (having a bar across the middle) which stood unemployed against the wall of my atelier, suggested to me a rough but convenient method of putting into

* This diagram, which is copied from the drawing that is attached both to the depositions of Dr. Gale and that of Mr. Huntington and Mr. Loomis before the Courts, is sufficiently minute, although defective in some details omitted, to serve for my own as well as their reference in describing the parts and operation of the first telegraphic instrument of 1835. [See next page.]

operation the printing or marking of the signs. I nailed it at the bottom against the edge of a common table. Across the lower part of the frame I constructed a narrow trough to hold three narrow cylinders of wood, ABC. A and C small, one on each side of the large cylinder B. The wooden clock D was placed at one end of this trough. The small cylinder C

Sheet. I.

FIG. 1.

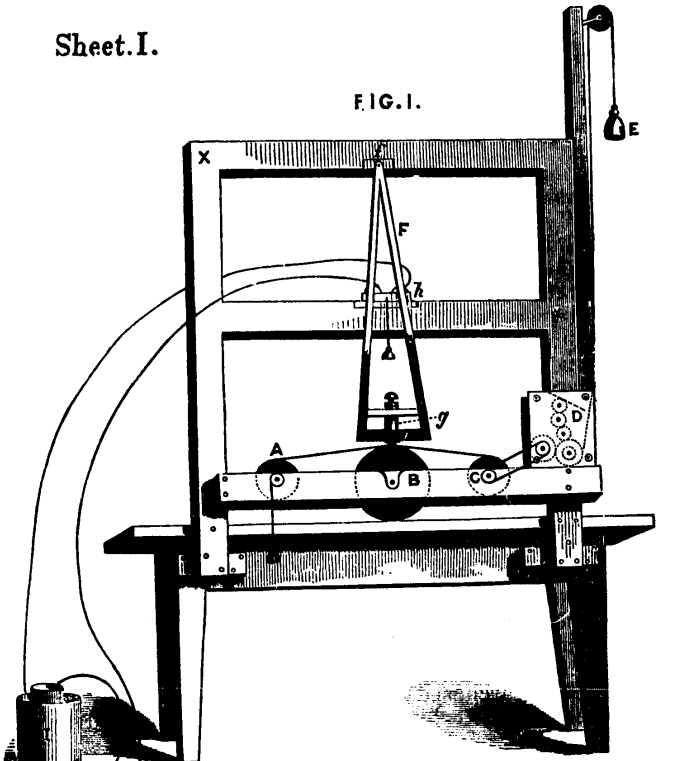


FIG. 2.

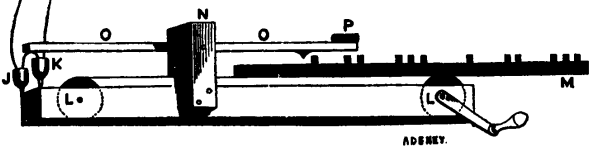


DIAGRAM 4.

Sheet.II. Fig.1.

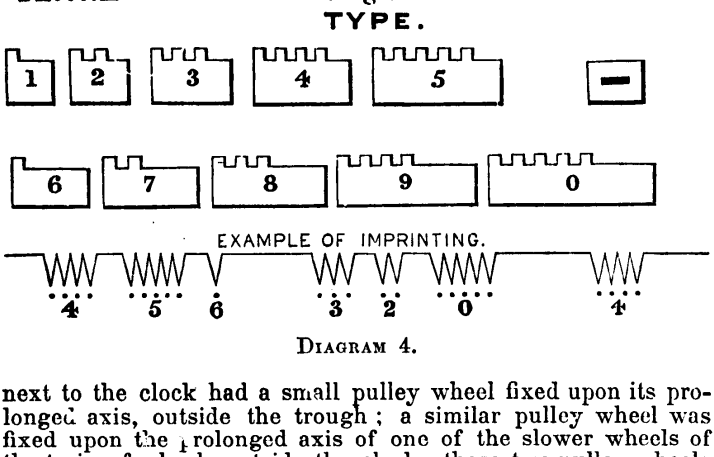


DIAGRAM 4.

next to the clock had a small pulley wheel fixed upon its prolonged axis, outside the trough; a similar pulley wheel was fixed upon the prolonged axis of one of the slower wheels of the train of wheels outside the clock; these two pulley wheels were connected by an endless cord or band.

Upon the other small cylinder A on the other side of cylinder B, was wound the ribbon of paper, composed of long strips of paper pasted together, end to end. When the clock train was put in movement, the ribbon of paper was gradually unrolled from its cylinder, and passing over the cylinder B. was rolled up upon the cylinder C by means of the cord and pulleys. To give the weight which moved the clock train a sufficiently long space in which to fall, a long rod or strip of wood projecting upwards was nailed to the side of the frame, at the top of which rod, was a pulley wheel over which the cord attached to the weight E was passed.

Upon the middle of the cross bar of the frame there was a small shelf or bracket *h* to hold the Electro-magnet, which was the moving power of the marking or printing lever.

The *Lever* was an A shaped pendulum, F, suspended by its apex at *f* from the centre of the top of the frame, directly above the centre of the cylinder B in the trough below. This lever was made of two thin rules of wood meeting at the top *f*, but opening downwards about one inch apart, and joined at the bottom by a transverse bar (which was close to the paper as it moved over the large cylinder), and another about one inch above it. Through the centre of these two bars a small tube or pencil case *g* was fixed, through which a pencil loosely played. The pencil had a small weight upon its top to keep the point in constant contact with the paper ribbon. Upon the lever directly opposite to the poles of the electro-magnet was

fastened the *armature* of the magnet, or a small bar of soft iron *h*. The movement of the lever was guided by stops on the frame at the sides of the lever, permitting to it only a movement forward to, and back from, the magnet; the pencil at the bottom of the lever was thus allowed to advance when the magnet was charged, and to retreat when discharged, about one eighth of an inch. The lever advanced by the attraction of the magnet, and retreated by a weight in the first attempts, but immediately afterwards by the action of a spring.

The first voltaic battery or pile* was of a single pair, I, having one of its poles connected by a conjunctive wire with one of the helices of the electro-magnet, and the other pole with *one* of *two cups of mercury* K; a conjunctive wire was connected with the other helix of the magnet. The only part of the voltaic circuit not completed was between the two cups of mercury J and K. When a forked wire upon the lever O O united the two cups J K the circuit was complete, the magnet was charged, the armature *h* was attracted, and the lever F drawn towards the magnet. When the forked wire was removed the magnet was discharged, and the spring brought back the lever to its normal position. When the clock work was put in motion the ribbon of paper was drawn over the large cylinder B; from the cylinder A, the pencil *g* on the lever, being in constant contact with the ribbon of paper, traced a continuous line lengthwise with the ribbon. When the lever was in its normal position, the line was upon one side of the ribbon as at *r*; when attracted by the magnet to the other limit of its motion, the line was on the other side, as at *s* in diagram [5].

The *pathway* of the pencil point (when the lever was attracted towards and held by the magnet for a longer or shorter time tracing the line *s*) contains the *three* elements of *points*, *spaces*, and *lines*, forming by their various combinations, the various conventional characters for *numerals* and *letters*. The other line *r* traced by the pencil when the lever is in its normal position may, therefore, be disregarded. Only the variations in the line *s* traced by the pencil when the magnet is charged is of importance. A specimen of these combinations is exhibited in the following diagram [6].

A is the line *r* in diagram [5] which the pencil traces when the lever is in its normal position.

B is the line *s* in the same diagram which is to contain the conventional characters to be read as if marked in points, spaces, and lines as on the line C below the ribbon of paper. The arrows show the direction of the movement of the ribbon of paper when the clock work is in motion.

* I had at this time a Cruikshank's battery of 12 pairs, but so out of order as not to be available for experiment.

Supposing the ribbon of paper in motion while the magnet is not charged, and the pencil to have commenced marking the upper line at A when arriving at *d* the circuit is quickly

DIAGRAM 5.

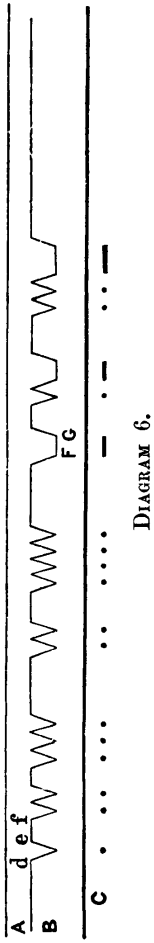


DIAGRAM 6.

closed and opened again ; the pencil is thus drawn a moment to the lower line B marking a transverse across and back again, leaving a *point* in the lower line B. But as the ribbon of paper is in motion, the transverse line back again does not return the way it came but goes back to *e*. From *e* to *f* is a

space. If the circuit be closed twice, and at each closing be opened quickly again, there will be two *points* left in the line B. followed by another space. If *three* times there will be *three points*, and then a longer *space*, and so on, making one

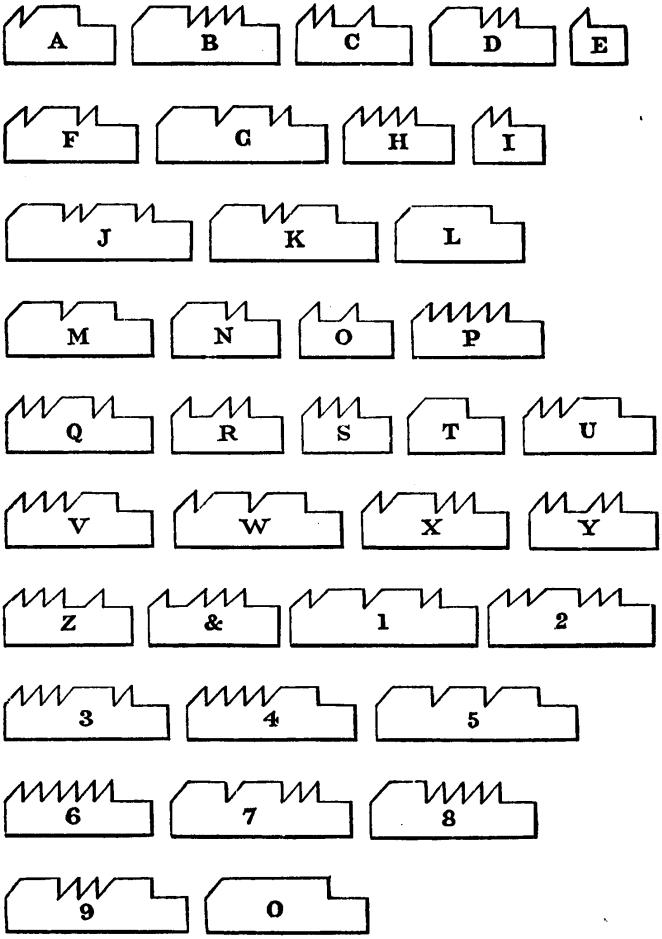


Diagram 7.

or more *points* and *spaces* at pleasure. But if instead of opening the circuit quickly, it be kept closed a moment, and then

opened, the pencil leaves a *line* on the line B, as at FG. Thus *points*, *spaces*, and *lines* are made at will. Combinations of these (strictly speaking *broken parts of a continuous line*) I made in sufficient variety to form my conventional Alphabet. See diagram [7].

At the time of the construction of this first telegraphic instrument, I had not conceived the idea of the present *key manipulator* dependent on the skill of the operator, but I presumed that the *accuracy* of the imprinting of signs could only be secured by mechanical mathematical arrangements and by *automatic process*. Hence the first conception, on board the ship, of embodying the signs in type mathematically divided into *points* and *spaces*. See diagram [2]. Hence also the construction of the type mould, and castings of the first type in 1832.

Having ascertained that the machinery, I had constructed, rude as it was, would move the ribbon of paper at a regular speed, and that the pencil lever was obedient to the closing and opening of the circuit, the next thing to construct was the manipulator or regulator of the closing and opening of the circuit.

I had already in abundance the type cast in 1832. These were now to be put to use.

I prepared rules or composing sticks M [diagram 4] of about *three* feet in length each, formed by two strips of wood, so placed side by side as to leave a narrow channel large enough to contain the type in desired order and to allow the cogs of the type to project above the upper edge of the rules. Through and along the bottom of the rules projecting downwards were several needle points about one-fourth of an inch in length; their use will be perceived presently.

A long trough LL, sufficiently wide to allow of easy passage of the rules through its length, was constructed with the following parts. Near each end of this trough were two small cylinders, of wood, LL. On the prolonged axle of one of them was a hand crank, and over the two cylinders an endless band of worsted tape about one and a half inches in width, which when the crank was turned, passed from end to end of the trough. Midway and across the trough was erected a small frame or bridge. N, within which a wooden lever OO was suspended parallel with the endless band, having its fulcrum at N at a point about two thirds its length, but the longer part reaching from the fulcrum to the end of the trough, on each side of which under the end of the longer part of the lever was placed the *two cups of mercury* J K. Upon the end of the lever and above the cups of mercury was fixed a forked wire so bent as to connect both cups when the end of the lever was depressed, and to disconnect them when it was

raised. At the other or shorter end of the lever a weight P overbalanced the longer part, and on the under side beneath the weight was a bevelled tooth projecting downwards. The rule or composing stick, having the type set up, was then placed upon the endless band; the needle points beneath the rule striking through the band and retaining the rule in its place. By turning the crank the rule was made to pass beneath the lever. The first cog of the type coming in contact with the tooth beneath the weight of the lever raised that end and depressed the other, causing the forked wire to descend into the two cups of mercury, and *closing* the circuit. When the cog had passed the tooth, the weight caused the tooth to fall into the space between the first and second cog, and the fork at the other end of the lever to rise out of the cups of mercury, *opening* the circuit. At each dip of the fork into the cups, the circuit was closed, the magnet was charged, the armature on the pendulum lever was attracted, and the pencil passed from the upper line A [diagram 6] to the lower line B. When the fork was raised out of the cups the circuit was opened, the magnet was discharged, and the pendulum lever with its pencil resumed its normal position by the action of the spring. A repetition of this process, as the rules with the type passed beneath the tooth on the lever, completed the action of the instrument.

This was the construction and mode of operation of the first recording instrument for *imprinting characters at a distance*. In this shape it "*produced a new practical result seen and felt and appreciated by the senses,*" witnessed, and testified to, by many witnesses as seen in operation in 1835, 1836, 1837. It was undoubtedly an *imperfect* instrument, but it produced, *then*, the same result that is produced more effectively, by more perfectly made instruments, *at this day*. It was a result never conceived nor accomplished before; it was an important practical result for the first time attained.

The recording instruments throughout the world at this hour have the same characteristics as this first rude instrument.

They *record* or *imprint conventional signs, points, spaces, and lines* upon a *ribbon of paper*, moved by clock *machinery*, and by the action of an *electro-magnet*, charged and discharged through the agency of *electricity*, by means of a *single circuit* of conductors. The mechanism of to-day is indeed more beautiful, more finished, more exact, and as varied in form as the varied forms of the timepiece. The result is consequently more rapidly produced, but the result is the same as in this original instrument. The *semaphore* was then transmuted into a *telegraph*. The *evanescent sign* had become fixed, permanently *written* or *imprinted at a distance*.

I have said that the modern instruments have the same characteristics as the first instrument.

To make clear the *identity* of the modern recording instruments with this first recording instrument of 1835, which at first blush may not be so obvious, I have made the diagrams [8, 9].

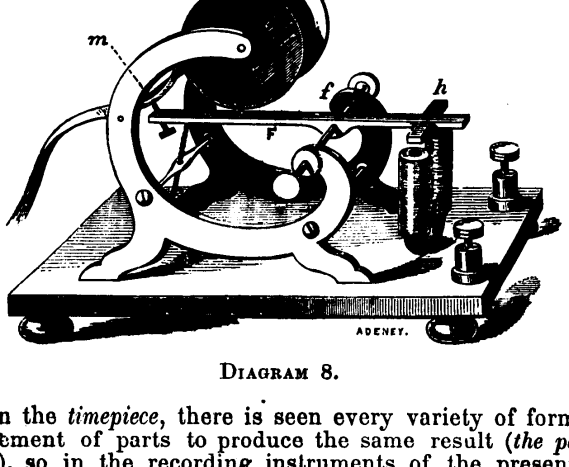


DIAGRAM 8.

As in the *timepiece*, there is seen every variety of form and arrangement of parts to produce the same result (*the passage of time*), so in the recording instruments of the present day there is the same variety of form and arrangement of parts to produce the *writing* or *imprinting*, the final result in all.

Compare diagrams [8, 9] with diagram [4]. The letters in each diagram refer to similar parts in each, so that in describing one, all are described. In diagram [8], the machinery that moves the ribbon of paper is removed in order the better to show the writing or recording apparatus. F is the lever; *f* its fulcrum; *h* the armature of the electro-magnet affixed to the lever; *m* shows the stylus or marking instrument in diagrams [8, 9,] affixed to the extremity of the lever, having the fulcrum *f* between the stylus *m* and the armature *h*. This is the modification in the modern instruments, whilst in diagrams [4 and 9] *g* shows the stylus affixed to the other extremity of the lever F, having the armature *h* between the stylus *g* and the fulcrum *f*. If, therefore, as in diagram [9], two ribbons of paper are put in movement, one before each stylus *g* and *m*, it will be seen that *g* in diagram [9] makes the zigzag marks, represented in diagram [6] like those of *g* in the original instrument, diagram [4], while at the same time, by the same movement of the lever, the stylus *m* at the other extremity of

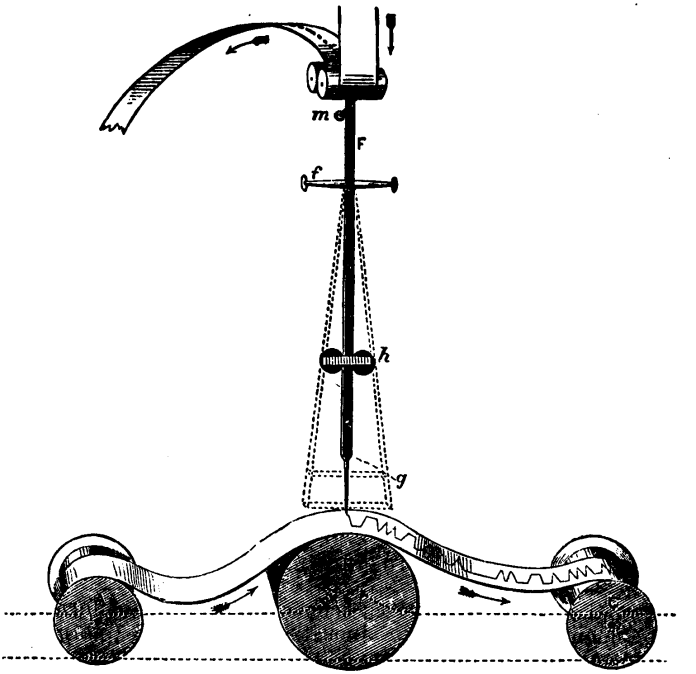


DIAGRAM 9.

the same lever, marks the alphabet in *points and lines*, or *dots and dashes*, upon its own ribbon of paper, the characters in universal use at the present day.

It is thus perceived that by prolonging the lever of the modern modification of the recording instruments beyond the armature *h*, towards the cylinder B, and affixing a stylus, pen or pencil *g*, on its extremity, and allowing it to be in contact with the moving ribbon of paper, as in the original instrument of 1835, the action of the lever F may be made to mark the original zigzag characters at *g*, while the modern points and lines are at the same time marked by *m* on its own ribbon of paper. The dotted lines shadow the original A shaped lever of diagram [4], showing the same assemblage and arrangement of parts as in the original instrument.

It may seem singular to some that the plan of direct up and down movement of the lever, as in diagram [8 and 10] at *m*,

to mark upon the paper (the plan devised on board the ship, and which is now the most universal), should not have been the first that was put in operation, since too it was the first and the most obvious mode devised. Having chosen, however, for economical reasons, the stretching frame as the most convenient support at hand for the machinery, it was necessary to adapt the parts to this choice, even if my results must be attained in a more indirect manner.

It is easy to see that the direct action of the lever, as at present universally used in the register, would accomplish the result better, and it was put into use almost immediately after the first trial. Lightness in the lever was a desideratum, and this seemed to be easiest attained by suspending it at its fulcrum *f*, but especially as a *pencil* was chosen as the first marking instrument (diagram 10), it was supposed to be ne-

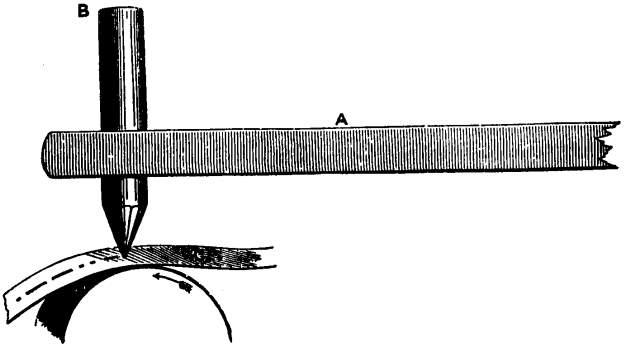


DIAGRAM 10.

cessary in some way to avoid the direct blow of the pencil upon the paper, which was produced by this mode, but which endangered the point, and therefore the zigzag sliding movement was adopted. The *pencil as employed* in diagram 9, at *g*, was not the only marking instrument devised and put in operation in the earlier instruments. Besides the direct action of the pencil as in diagram 10, *fountain pens* of various kinds, one of which is shown in diagram 11, and a small *printing wheel*, as shown in diagram 12, were used, the latter being supplied with ink from a sponge with which it was in contact. All these were used with more or less of success (see doc. VI., Appendix).

The same *result*, however, to wit: *recorded characters* representing numerals and letters, and words and sentences, was given by each of these modes in this first constructed instru-

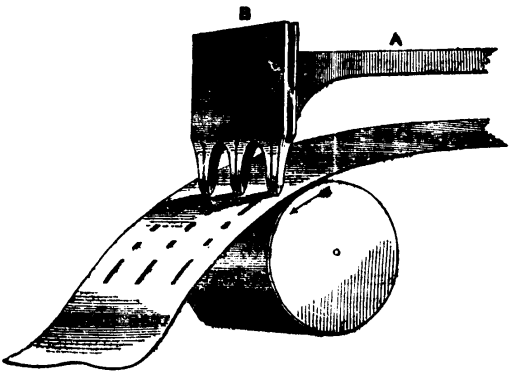


DIAGRAM 11.

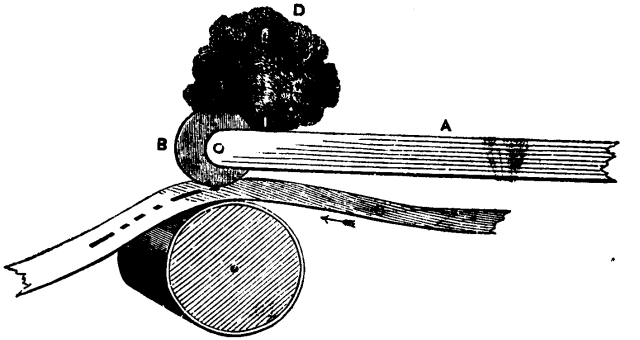


DIAGRAM 12.

ment, as is given in instruments of the present day. The instrumentalities are the same, and the result the same, the only difference is in the mode of using the *marking* lever.

It will be now perceived that my invention of 1832 had certain very important novel characteristics which distinguished it from all inventions of a previous date. It was not like any of them.

Although the contemplation of static electricity as a means of producing a permanent record at a distance gave rise in my mind to the conception of the invention on board the ship, it was not the *static* form of electricity, but its *dynamic* form, which I immediately adopted for carrying into operation what I had devised. Electricity was proposed to be used by me

neither in the form, nor for the purpose, nor by the same instrumentalities, as were proposed in the earliest contrivances, say previous to the year 1800. None of them proposed to *record* their intelligence. None of them proposed, or made use of the *electro-magnet*, for it was not then invented, nor the scientific basis of it discovered. None of them had invented a *system of signs* adapted to recording, for the necessity for them had not arisen. For the same reason, none had proposed a *moving ribbon of paper* for receiving the record. None proposed to use *a single circuit of conductors*.

In the earliest attempts to use electricity, for communicating at a distance, *static* electricity was the form proposed and attempted. Failure was the uniform result, not for the lack of ingenuity in the savans of that day, but from the intractible nature of the form of electricity to which they were limited. When the *dynamic* form of electricity became known, it was at once seized upon as an agent in accomplishing communication at a distance.

Still between the years 1800 and 1832 the means by which that end was to be accomplished were all *semaphoric*. *Decomposition* by dynamic electricity in the form of *gas bubbles*, and the *deflection* of the *magnetic needle*, were the sole novelties in the signals of their proposed plans. No period, therefore, is more strongly isolated from all previous dates than the date 1832 as the epoch of a *new method* of applying electricity by the *electro-magnet* to the *creation of a* NEW ART, of a *new method* of communicating to a distance, to wit: *recording*, a method wholly unlike any previously imagined or invented.

But the instrument I had devised in 1832, and constructed in 1835 (so far at least as to demonstrate its practicability to communicate *from* one station *to* a distant station), did not completely embody my *whole plan*. This *whole plan* was not complete until I could, by a *duplicate* of the instrument, have the means of a return from that distant station. This was necessary in order to *receive from*, as well as to *send to*, a particular station. The *whole plan* comprized intercommunication or reciprocal communication.

It is true that any ordinary mind could easily comprehend from the operation of the single original apparatus that, if precisely the same apparatus were used from the *receiving* station to the *sending* station, *intercommunication* would be complete. No new appliances were necessary. A duplicate of the instrumentalities already in use from the *sending* station to the *receiving* station was all that was needed to complete my whole plan, and to establish intercommunication. But this was an affair of finance, and not of invention. To supply the duplicate required pecuniary means, and these I had not at command. But the rigidly captious may ask, "Why did you not

borrow the pecuniary means?" My reply must be that I preferred the delay, and the hazards of the delay, to the hazard of being unable to repay a loan. I must be pardoned if I state that, even from my earliest youth, I ever had the deepest repugnance to incur debt by borrowing, even from my own relatives. Is it my idiosyncracy? If so, the reader will excuse it, and my allusion to it.

By dint of the most rigid economy, I was able slowly to complete and to add this duplicate, necessary to complete my *whole plan*. Although the original single instrument was freely shown to my pupils and to many friends, I was reluctant to make any more public exhibitions of the invention until this duplicate should be added, and this was done in the early part of August, 1837.* Early in September, I was more free in exhibiting the invention, and on the 2d and on the 4th of September I showed the instruments in operation to some hundreds of persons assembled in the large hall of the University. Most writers on the telegraph choose to take this date as the date of my *invention*. But why, with the facts before them, is this just? To the existence and previous operation of the essential part of my whole plan, long before this more public operation of 1837, there were many witnesses† whose evidence is before the Courts on oath. But there are other writers who, having ascertained the date of my *Caveat* at the Patent Office on the 6th October, 1837, and others again who find the date of 7th April, 1838, the date of my application in Washington for Letters Patent, who choose to consider this latter date as the date of my invention.‡ To all these I propose a question. Suppose I had never applied for Letters Patent for the invention, but had chosen to give it to the public, or suppose I had never brought it to France, would there, therefore, be no invention, and no inventor of it? Their answer will settle that point.

Between the date, 1835, of the completion of the first instrument and 1837, the date of its more public exhibition, there was a very important addition to it, which I had already devised and provided against a foreshadowed exigency, to meet it if it should occur when the conductors were extended, not to a few hundred feet in length in a room, but to stations many miles distant. I was not ignorant of the possibility that the electro-magnet might be so enfeebled when charged from a great distance, as to be inoperative for *direct* printing. This possibility was a subject of much thought and anxiety long

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* Supreme Court Record, page 176.

† See documents in the Appendix.

‡ There is still another, the Comte du Moncel, who, assuming the date of *my bringing the invention to France in* 1838, gives 1838 as the date of the invention.

previous to the year 1836, long previous to my acquaintance or consultations with my friend Prof. Gale on the subject, but I had then already conceived and drawn a plan for obviating it. The plan, however, was so simple that it scarcely needed a drawing to illustrate it; a few words sufficed to make it comprehended. If the magnet, say at twenty miles distant, became so enfeebled as to be unable to print *directly*, it yet might have power sufficient to close and open another circuit of twenty miles further, and so on until it reached the required station. This plan was often spoken of to friends previous to the year 1836, but early in January, 1836, after showing the original instrument in operation to my friend and colleague Prof. Gale, I imparted to him this plan of a relay battery and magnet to resolve his doubts regarding the practicability of producing magnetic power sufficient to write at a distance. To this fact he testifies in his deposition before the Courts. [See his deposition in the Appendix, doc. VI. and diagram 4, sheet II.]

This apprehended difficulty of an enfeebled magnet, as distance increased, was among the very first subjects of discussion with Prof. Gale; so soon as my plan for obviating it was revealed to him it was deemed perfectly satisfactory. It was not then permanently embodied for use. A moment's reflection will show why. The relay was not then necessary to show the final result of the telegraph in the short circuit of less than a mile arranged around a hall. The operation and result of printing at a distance was complete without it. But the frequent objection made by visitors that the instrument shown them might answer well enough for an interesting philosophical experiment in a class room, but would not operate at a distance, at length induced me not merely to explain the *relay* by words and diagrams, but, so soon as I could command another magnet, to embody it in proper form.

Prof. Gale's clear deposition before the Courts describes the time and manner of its temporary construction. It was in March, 1837. This plan of the relay thus made in the spring of 1837 was productive of an important incident of great consequence to me in the prosecution of my invention. A few days after the more public exhibition of the telegraph the late Alfred Vail, Esq. (then a student in the University, who was present at the Exhibition on the 4th of September), became so fascinated with the invention that he called to have it more perfectly explained to him. The usual objection that it would not operate at a distance was a bar to his belief in its success.

No sooner, however, had I explained the operation of the relay than he desired an interest in the invention and, to procure this interest, he offered to negotiate with his father and brother to supply the funds necessary to have constructed

such a telegraphic instrument as would demonstrate to the United States Congress, and to the country, its practicability and utility. Thus the invention of the relay was the immediate cause of the construction of the apparatus which was shown to Congress in Washington in the winter of 1837-1838.

Some European writers on the Telegraph have recklessly ventured to charge me with appropriating the relay from Prof. Wheatstone. In the *Moniteur* of February 14th, 1865, the charge takes this shape.

"En 1838 l'American Morse arrive en Europe pour bréveter un nouvel appareil qui n'était autre que celui de Steinheil perfectionné, et ne pouvait se prêter a une distance quelque peu considerable. Sentant le besoin de combler cette important lacune, Morse se rendit en Angleterre, *pénétra l'idée des Relais* de Wheatstone, et en revient avec un appareil à peu près complèt, qui est le plus universellement employé de nos jours."

This statement is so full of errors that, had it not appeared in a journal of such distinguished position as the *Moniteur*, I should not deem it necesary to expose them. It is not true that my apparatus was no other than Steinheil's improved (if it is meant by this assertion to intimate that my invention was *subsequent* to Prof. Steinheil's). It was prior by some years. It is not true that my invention had not been extended but to a short distance. It had operated through ten miles before I left America for Europe. [See my letter to the Secretary of the Treasury of November 20th, 1837.] It is not true that "I went over to England from France in 1838 full of the idea of Wheatstone's relays, and returning to France attached them to my apparatus in order to fill an *important gap* and so to complete my system." The "*important gap*" had long before been filled. I had brought with me from America in the spring of 1838 not only my own relay, but a copy of my American specification and description of it, the original being left in the archives of the American Patent Office, which specification and description were translated nearly verbatim in my French Brevet, which was registered *seven months* before I went to England from France. It was a relay not only unlike Wheatstone's, accomplishing that which his could not accomplish but having a different *purpose*, a different *construction*, and a different *result;* invented in 1836 (so far as drawings prove invention); embodied and shown in operation in March, 1837; and specified at the Patent Office in Washington in April, 1838, and all these dates, be it observed, are previous to my leaving America for Europe with the invention.* Supposing

* This charge, with other mistakes, was promptly replied to at the time in the

the two relays, however, to have been identically the same, was it necessary to charge that one of the two inventors of the same thing clandestinely robbed the other?

Because my relay was invented prior to Professor Wheatstone's, does it follow that he borrowed his from mine, especially when the two are wholly unlike? Dates show I could not have borrowed from him. I never charged that he borrowed from me.

Since I find, however, in many works on the telegraph the same imputation of my having borrowed the relay from Prof. Wheatstone, suffer me to show the correctness of my assertion that the two relays are unlike.

It is capable of perfect proof that Prof. Wheatstone had no relays in April, 1837; that when he contrived his relay it had for its specific and only *purpose* the "*sounding of alarums, without giving any other signals than such alarums.*"* This is the language of his specification. The *construction* for this purpose was by a needle having a small fork upon one end which, when deflected and dipping into two cups of mercury, closed a second circuit connected with the helices of an electro-magnet, which closing of the circuit released the detent of an alarum apparatus, thus calling attention to the signal apparatus, and this was its only and *final result.* The *signal* part of the apparatus was entirely distinct from this alarum.

My relay, on the contrary, had for its specific *purpose* the propagation or propulsion of a sign to be finally *imprinted* at a distance. The *construction* was by an electro-magnet at the far terminus in a first circuit closing a second circuit, this second circuit having in it in like manner an electro-magnet, which second magnet closed if necessary a third circuit similar to the second, and so on until the last electro-magnet of the series imprinted the projected sign upon paper, which *imprinting* was its final *result*.

Thus, even if I had examined or heard of Prof. Wheatstone's relay before I had invented mine, there was nothing either in its *purpose*, *construction*, or *result* that I could wish to appropriate: nothing which I could have used to accomplish my result. It will, moreover, be seen that Prof. Wheatstone's result could easily be accomplished by my relay.

If previous to the year 1832 all the so styled telegraphs were *semaphores*, and none of them were strictly *telegraphs*, I

Moniteur by an amiable and learned friend, the Chevalier Baudouin, and refuted, so far as he was acquainted with the facts, and I have recently been gratified to know that the author of the charge made it in honest ignorance of the true state of the case.

* Wheatstone's Patent. *Repertory of Patent Inventions*, vol. II., January, 1839, page 96.

proceed to ask attention to the character of the inventions *subsequently* introduced for communicating at a distance.

In 1833 Baron Schilling of St. Petersburg made some ingenious essays to construct an electric semaphore, and, following the suggestions of the distinguished French savan Ampère of using the deflection of the needle to produce the signals, he appears to have been the first to have made direct experiment of the needle system and attempted to put into practice Ampère's suggestion.

But Baron Schilling's apparatus, ingenious as it was, was a *semaphore*, complicated, and (without much modification) was not practical, and does not appear to have been put into practical use.

The illustrious German savans Messrs. Gaüss and Weber in 1834 communicated signals through electrical conductors, by means of the deflection of the needle, at Gottingen between the Observatory and the Cabinet of Natural History. This system was also a *semaphore*.

In December, 1836, the ingenious andhigh-minded Steinheil (whose name I cannot mention without a feeling of the highest respect), proposed a method of strictly *telegraphic* communication. He was occupied with the design of transforming the hitherto *semaphoric needle system* into a *telegraphic needle system*. He proposed to *record* or *imprint* by the touch of the deflected needle. He was the first in Europe who conceived,(and independently), the idea of the possibility of a recording or printing telegraph, as he was the first magnanimously and unselfishly, to prefer and recommend to the nations my system of permanent recording by means of the *electro-magnet*, and thus abandoning his own needle telegraph, he generously and spontaneously used the influence of his high position as Director of the German Telegraphs to cause my system to be adopted throughout Germany and Switzerland, and eventually throughout Europe, and I may say the Eastern Continent, as it already had been adopted throughout the United States. To Prof. Steinheil, however, exclusively belongs the discovery of the availability of the earth as a conductor of Dynamic Electricity a discovery of the greatest importance to the telegraphic enterprizes of the age, of commercial and economic advantage, and for which Prof. Steinheil has not as yet received the reward which is his due.

In June, 1837, Messrs. Cooke and Wheatstone in England improving upon the needle system of Ampère and Schilling, had so far completed their *needle semaphore* (the most perfect at that time of all the systems of that class) that it was soon after established throughout the British Islands, and to some extent elsewhere.

Although it has yielded to telegraphic and other systems,

considered more efficient in all other countries, it is yet in operation extensively in the British Islands.

An unhappy contest between these two distinguished gentlemen has arisen which makes it a matter of some delicacy for one in my position to submit any remarks upon their inventions; yet, having the highest personal regard for both of them, but in a special degree for the noble and magnanimous character of Mr. Cooke, I may venture a few remarks upon the antagonism which so many writers assume to exist between the inventions of Messrs. Cooke and Wheatstone and mine, and which is made the basis of a contest for priority, a contest which one of the clever writers on the subject [Dr. Wynter] has said is "still unsettled and constantly renewed."

This presumed antagonism has no real foundation in fact, as I think I can easily demonstrate. The misuse of the words *telegraph* and *semaphore*, as I have already shown (that is, the misapplication of the former name to the processes and results belonging to the latter) have undoubtedly tended to confuse the judgment, and keep alive the presumption of antagonism, because of an assumed similarity of systems essentially different occasioned by a misapplied similarity of name. If the applicability of the names chosen to designate the *things themselves* had been duly considered, the illusion of similarity would have been dissipated. The *semaphoric* and *telegraphic* systems would have been perceived to be essentially different.

The *Semaphoric conveys* an *evanescent signal.*

The *Telegraphic writes* or imprints, a *permanent sign.*

The system of Messrs. Cooke and Wheatstone is *Semaphoric.*

The Morse system is *Telegraphic.*

The Semaphoric requires only the means of *showing a sginal,* and this is accomplished in Cooke and Wheatstone's invention by various deflections of the *magnetic needle.*

The Telegraphic requires the means of *writing or imprinting the sign permanently,* and this is accomplished by *chemical decomposition,* and also by the mechanical power of the *electro-magnet.*

In the semaphoric invention of Cooke and Wheatstone the *electro-magnet* is not used for conveying signals.

In the telegraph of Morse the *deflection of the needle* is not used for writing or imprinting signs.

That which is common to both systems is the final result, *communicating at a distance,* but this is a result that has been accomplished by various modes from time immemorial, and is not in dispute, so that we are limited to the consideration of *modes* of producing this final result.

Two modes are before us, a Semaphoric mode professedly different from, and an improvement upon all previous sema-

phoric modes, and a Telegraphic mode, professing to be the inauguration of a *New Art*, not the modification or improvement of an old telegraphic mode, but the invention of the telegraphic mode itself.

Does not this view of the case show clearly that Messrs. Cooke and Wheatstone may have elaborated their improvements and perfected their system wholly independent of the means that I have used in inventing my new art; and is it not as clearly shown that my system may have been perfected wholly independent of the means Messrs. Cooke and Wheatstone have used in their system?

If this then is the true state of the case, is it not evident that the two inventions, the one semaphoric and the other telegraphic, are distinct from each other, and consequently that each system may have its original inventor, the one independent of the other?

Where then is there any ground for the antagonism which writers have assumed to exist? It is not in the inventions themselves for they are *not alike*. It has arisen from the groundless but assumed charges made by the historians of the telegraph of borrowing or otherwise appropriating by one, especially by Morse, of the labors or contrivances of the others.

Some of these charges I may therefore notice, especially those which *seem* to be supported from some of the expressions of Prof. Wheatstone himself.

It has been charged that but for the experiments of Prof. Wheatstone resulting in the discovery of the velocity of electricity, and some of the laws of the electro-magnet the telegraph could not have existed.

It was perceived by the philosophers of a past age that Electricity was so rapid in its passage through a conductor that, in the attempt to measure the speed through many miles, it was pronounced to be *instantaneous*. If not a fact it was a plausible hypothesis. It was for a long period assumed to be a fact, and it formed the basis of various attempts to construct an Electric Telegraph. That modern philosophers should have ascertained, as Prof. Wheatstone has done, by more exact and ingenious methods of measurement that the rate of its passage was not instantaneous, but is some 200,000 miles a second, or that it has the speed of light, is indeed a beautiful discovery, nor would I be thought to attempt to detract aught from its scientific importance nor form the deserved honor of its ingenious discoverer.

It is, nevertheless, evident that while the discovery may be of practical use for other purposes, it does not affect nor modify the invention of a telegraph based on the formerly received hypothesis of the *instantaneity* of electrical transmission.

Prof. Wheatstone attributes I think erroneously the failures of the previous attempts of "Cavallo, Reiser, Soëmmerring, Ronalds, Ampère, and others" to "the imperfect knowledge then possessed respecting the velocity and other properties of Electricity." In this he is mistaken so far as velocity is concerned. Whether the velocity of electricity was "infinite" as some supposed, or was limited to the speed of light (200,000 miles in a second), is a question the solution of which could not affect a use of electricity adapted to a telegraph whose utmost limit of action is comprised within 25,000 miles.

While I most cheerfully concede to Prof. Wheatstone great profundity and success in his scientific researches, I must contend that I have seen no evidence that any investigations of the laws of observed facts, and consequent discovery which he has made, is *essential* to the successful establishment of a *telegraph* or a *semaphore.* I have admired his beautiful researches in electrical science, his ingenious and successful appliances for ascertaining the velocity of electricity, which is his most brilliant discovery in that branch of science, but I fail to perceive that the discovery of the velocity of electricity has furnished a single advantage, over the assumed *instantaneity* of its passage, the previously received hypothesis since the days of Franklin and Watson. It is certain that in the process and means I employed in my invention, based as they were on the hypothesis of the *instantaneity* of the passage of electricity, there has not been the slightest modification made or needed in my invention in consequence of Prof. Wheatstone's discovery. The process and means were devised and put in successful operation long before Prof. Wheatstone's experiments on the velocity of electricity. An illustration will serve to make it clear that the result of his experiments could not affect my invention.

I wish, for example, to send daily important messages to a friend living twenty (20) miles from me, they must be delivered in an hour. I must therefore find a courier qualified for the duty, by his ability to travel that distance, in that time. I am referred to one who has the reputation of being able to travel at the extraordinary rate of one hundred miles an hour. He is just the man. For if he can go 100 miles in an hour, he can certainly go 20 miles in the same time. I take him into my employment, and he readily accomplishes his daily task. After some time an ingenious friend questions the ability of my courier to travel 100 miles an hour, and on careful trial and computation ascertains that he can only travel sixty (60) miles an hour. What then? my reply to such an announcement is, "as I require but *twenty* miles an hour of what consequence is it to me, for the use I make of him, to know whether he can travel one hundred or only sixty miles in that time?

The distance to which I need send by telegraph is at the utmost limit 25,000 miles. Franklin says that for this purpose electricity is my courier, because its speed is *instantaneous*. In view of this assumed fact I construct an apparatus capable of sending telegrams around the globe, or 25,000 miles in an instant of time. Prof. Wheatstone now appears, and most ingeniously and beautifully demonstrates that electricity is not *instantaneous*, but that it travels only 200,000 miles in a second. Now as this is eight times greater than the distance that can ever be required, if it is proved that my apparatus is *incapable* of operating at the *infinite* distance suggested by the hypothesis of the *instantaneity* of Electricity but is still capable of operating with a speed of 200,000 miles in a second, this limitation cannot render ineffective instrumentalities for operating one eighth of the distance.

If, therefore, Prof. Wheatstone's discovery had never been made, it is clear that my invention would remain for all telegraphic purposes as effective as ever.

But is it said that although his discovery of the velocity of electricity did not make necessary any modification of my telegraph, yet his other researches on the laws of electro-magnets were essential to produce telegraphic recording at a distance? This also is an error. I find in Professor Wheatstone's statement of his case an incident and a *date* which decide that point. On October 26th, 1840, he addressed a letter to Mr. Cooke announcing his determination "to carry out his *future* investigations alone." He then proceeds to say, "*After* this resolution had been taken" (that is *after* October 1840), "I commenced a series of researches on the laws of electro-magnets, and was fortunate enough to discover the conditions which had not been made the subject of philosophical inquiry, by which effects could be produced at great distances. *This rendered electro-magnetic attraction for the* FIRST TIME, *applicable in an immediate manner to telegraphic purposes.*" This is an extraordinary announcement. Surely the operations of nature do not wait for the philosopher to investigate and determine the laws of their action before they can act. An ingenious man, for example, is first to observe that a magnet attracts iron and not other metals, and he therefore applies the magnet to separate iron filings from other metallic filings. His invention is a success, whether he professes or not any knowledge of the laws of magnetic attraction. The inventor, indeed, in this case, preceeds the philosopher who is engaged in educing these laws. The inventor supplies him with a fact, one operation of the hidden law. It is this *fact precedent* that furnishes to the philosopher a clue to the discovery of the law.

The law acts independently of inventor or philosopher. It

is the *fact* that led to the *discovery* of the *law*, and not the *discovery* of the law that has produced the fact.

Prof. Wheatstone appears not to be aware that a successful application of "electro-magnetic attraction to telegraphic purposes" had already been made some *three* years before he commenced his investigations. Whatever other results therefore may emanate from his subsequent investigations, they cannot obliterate the fact of a *previous* and successful application of the electro-magnet to "telegraphic purposes," and this too at "great distances."

In the Congressional Records at Washington is a letter of mine to the Secretary of the Treasury, of the date 28th, November, 1837, which contains this passage. After alluding to a previous letter in which I had informed the Secretary that "I had succeeded in marking *permanently* and *intelligibly* at the distance of *half a mile*," I proceed to say: "At a distance of *Five miles*, with a common Cruikshank's battery of 87 plates, the marking was as perfect on the register as in the first instance of *half a mile*. We have recently added *Five miles more*, making in all *Ten miles*, with the *same result*; and we have now no doubt of its effecting a *similar result* at any distance."

It may be well here to inquire what known facts were already in existence at the time of my invention in 1832 necessary to produce the telegraph.

There was the fact that electricity travelled with a velocity so great that philosophers pronounced it instantaneous.

There was the fact that static electricity, too erratic, too uncontrollable for telegraphic or semaphoric use, had been superceded by dynamic electricity.

There was the fact of many modifications of the voltaic battery in common use (the Cruikshank's battery among others), and it was well known that the intensity of dynamic electricity was increased or diminished within certain limits in proportion to the number of elements put in action.

There was the fact of the electro-magnetic discovery of Oersted, which led to

The multiplier of Schweigger,

The electro-magnet of Arago,

The improvement of the electro-magnet by Sturgeon, Dana, Pouillet and others.

There was clock machinery ready for adaptation to the regular movement of a strip or ribbon of paper.

There were levers, with pens, pencils, points or printing wheels for marking.

There was wire for conductors, and non-conducting substances for insulation.

What else was necessary to be added to this catalogue of known facts to construct a telegraph? One other fact only

was wanting, and that was, a *system of signs adapted to the capabilities of the mechanism for printing at a distance*; and this *system of signs* I invented in 1832, and adding it to the inventory of known facts successfully combined them to produce the telegraph.

Whatever new facts, therefore, Prof. Wheatstone may have discovered in his researches into the laws of electro-magnets after 1840, it is evident that sufficient facts were known in 1835, and even in 1832, to enable me successfully to apply them "in an immediate manner to telegraphic purposes," and having shown my system of telegraphs in operation some years before he commenced his researches, it is impossible that I could have derived any aid from the results of his investigations at the period of my invention.

Some confusion has been caused by confounding the *time* of the *invention* with the *time* of its *practical establishment*. It surely needs no argument to show that an *invention* may exist, indeed, must exist, *before* its *practical establishment*. The one exists by the will of the inventor, the other is dependent on other wills, and must wait the slower concurrence of other circumstances, financial and administrative, and the real or supposed needs of the public.

In my critique, however, on Messrs. Cooke and Wheatstone's system of communication, I trust no one will charge me with the design of undervaluing their genius and labors in accomplishing for Great Britain the *introduction* of their system so widely and so effectively in their own country.*

I have believed that the system which they have so admirably perfected (being a *semaphore*) and mine (being a *telegraph*) are so unlike that there is in truth no question of interference from similarity. It is only since I have been charged by others, not by them, with borrowing my invention from them, that I have deemed it my duty to myself and to my friends to demonstrate the groundlessness of the charge.

The simple and effective instruments as modified by Messrs. Digney Frères, of Paris, embody the distinctive features of my invention more to my satisfaction than any of the French instruments. There is a modification which they have made, however, which requires a few remarks to prevent misappre-

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* As to the honor, however, of *first introducing into Great Britain*, and *establishing*, the practical ELECTRIC SEMAPHORE, I believe there is now no longer a question that it is due to the zeal, perseverance, and eminent skill of the ingenious *William Fothergill Cooke*, who is declared by the *award* of those distinguished savans, the late Sir Isambard Brunel and Professor Daniel (to whom, as arbitrators, was referred the question for decision, both by Mr. Cooke and Professor Wheatstone), to *stand alone* in this position, and it is worthy of notice that this award is accepted and signed by both the referring parties.

hension in regard to its exact nature. In reading the excellent work of M. Breguet, p. 163, in his chapter "*Morse Register marking the signs in ink*," "*Recepteur Morse faisant les signaux à l'encre*," I find some things to correct. A wrong impression is made in describing the mode of *embossing* the characters by a *steel point*, "a gaufrage," as if that were my *only* original mode of marking. This is not the fact; *a pencil*, a *fountain pen*, and the *small printing wheel* by which ink was used, were among the first modes of marking. See diagrams [10, 11, 12,] and extract in Appendix, doc. VI. There were many modes of marking which I devised and tried, but experience alone could settle which was best; the pencil and pen and small printing wheel with ink were the original modes in use; the steel point [diagram 13.] for embossing the character was

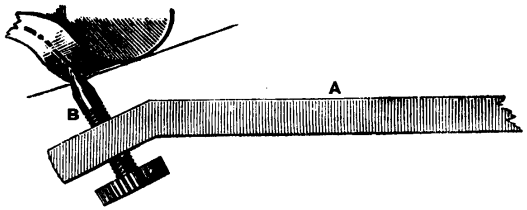


DIAGRAM 13.

invented some time after, and patented as an improvement, since it dispensed with ink; M. Breguet gives to Thomas John, of Prague, the invention of the *small printing wheel*, "une molette ou roulette," to mark the characters, and states that he received for his invention a platina medal from the Society of Encouragement.

That Mr. Thomas John made his improvement independently, without a knowledge of the fact that I had it in use nearly 20 years before, I have no doubt; but it is nevertheless true that the introduction of this inking wheel is not a novelty; whatever of novelty there is in its present use consists in the *mode* of its application and in the beautiful apparatus constructed by Messrs. Digney Frères the mode, so far as I know, is new. The mode of application of Mr. John is different from mine and from Messrs. Digneys'. My original mode (its first use) was by *bringing down* the printing wheel, inked from a sponge, upon the paper. Mr. John *brings the printing wheel* against the paper *from the side*.

My *caveat*, filed in the Patent Office in Washington on the 6th of October, 1837, in describing the Register specifies: "3d, A *pencil*, or *fountain pen*, or *a small printing wheel*, or any other

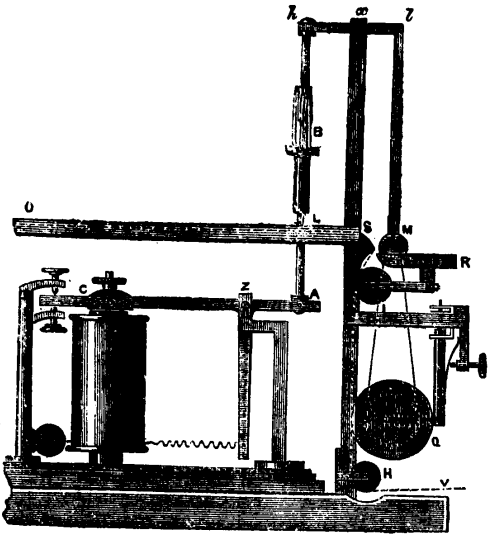


DIAGRAM 14.

marking material," and the mode of using the wheel is also described, thus: "when the *printing wheel* is used, the *wheel* is *brought in contact with the paper* by the magnet when required to mark." The wheel in my first experiments inked by a sponge was *brought down* upon the paper; Mr. John's mode of applying the wheel, inked in a reservoir, was by bringing it *against the* paper from the side, while Messrs. Digneys' mode was *bringing the paper* against the wheel inked by a felt roller. This latter mode I conceive to be a substantial improvement, since it combines delicacy with efficiency, and requires so much less power for the operation that even the relays can be dispensed with on lines of considerable extent. My original mode of using the *printing wheel* by ink from a sponge I found so inconvenient, from its constant tendency to soil the paper, and the fountain pens of every variety of form so unreliable, that the *steel point* "à gaufrage" [diagram 13.] I considered at the time a great improvement, since it gave the characters with certainty, without the inconvenience of constant attention and the dirt and accidents of the ink.

If, then, judged by the first rule laid down by Dr. Russell, I claim to be the *inventor of the first recording telegraph* (not to say, strictly speaking, the *first real telegraph*), am I not "the

first who produced the practical result which, however imperfect, gave a result which was seen, and felt, and appreciated by the senses?" Am I not, according to this rule, "the true maker and inventor whom the world should recognise, no matter how much may have been done by others to improve my work?"

Let me not be misunderstood as appropriating to myself the credit of the many modifications of the telegraph that have since been made in every part of the world, because I claim the invention of the *generic telegraph*. I do not pretend that the mechanism of the first forms of the telegraph was not rude, and even uncouth when compared with the beautiful workmanship of the European ateliers, of the hundreds of accomplished mechanicians who have brought to the work their incomparable ingenuity and skill, but I think I may justly claim that the essential characteristics of a *new art* were demonstrated even in the rudest instruments, constructed in the earliest times of the invention. So suggestive were the novelties introduced by the promulgation of the *new art*, so wide the field which it opened for investigation in science and mechanics, that it would be strange indeed if modifications of the separate elements that made up the whole invention should not at once be conceived and produced. And yet I may appeal to the fact generally acknowledged that the essential features of the original invention have not been obliterated; they can be easily and distinctly traced through all the improvements made in the various parts by which the different processes of the art have been more easily performed.

No one can view the beautiful modifications of the recording telegraph displayed in the present splendid Exposition in the Champ de Mars by Digney Frères, by Siemens and Halske, by Caselli, by Hughes, by Le Noir, by Breguet, by Hipp, by Leopolder, by Arlincourt, and by numerous other most ingenious men of various countries, without fully subscribing to the justice of the latter part of Dr. Russell's rule, to wit: that "each of these improvers after his kind is deserving of recognition for what he has done." I can conscientiously say that no one has seen the improvements and modifications by these distinguished men with more sincere gratification and admiration than myself, and no one more cordially accords to them the honor which is their due.

I have thus given a somewhat minute detail of the character and operation of the *first telegraphic instrument of 1835*. It must not be inferred, however, that the *form* in which it was first tried was the one I proposed for adoption in practice without modification, nor that it was long left in that rude condition, nor that the subsequent improvements were left to be made by others. The first instrument was intended to

demonstrate the possibility of *writing at a distance* and a *process* and *means* by which that result could be attained; and the demonstration was a success. It was the *demonsiration* of the *new art*. It had manifest defects of mechanical construction, defects which, after the first demonstration, were quickly remedied, delayed only by *financial* not by *mechanical* or *scientific* obstacles. I was soon enabled to employ skilled mechanicians who made for me new instruments of various forms and arrangements from drawings and plans which I furnished to them; yet all the new instruments possessed the same essential characteristics as the first instrument.

The facts brought out in this history of the *earliest*, the *generic telegraph*, will enable the reader to correct many of the erroneous attributions (in the works of writers on the telegraph) of certain telegraphic instrumentalities to the wrong authors. There is, nevertheless, one instrumentality to the origin of which it is necessary to direct special attention, since such incorrect notions of its origin have been so strangely conceived and promulged by a telegraphic historian in America (Mr. Prescott). Mr. Prescott may naturally be supposed to be properly informed on the subject, having around him documents at his command which should have prevented his commission of such gross mistakes. I allude now specially to his account of the instrument called, in America and in England, the *sounder*, and in France, *parleur* (a *speaker* or *talker*).

Mr. Prescott, at page 419 of his work, thus recklessly dogmatises: "*Adaptation of the sounds produced in making dots and lines to audible telegraphy.* Reading by *sound*, so far as the Morse telegraph is concerned, was purely an *after thought*, taken up by the *operators of their own accord*."

Again he says, "The credit of *introducing*, as well as the *discovery*, as far as telegraphing goes, is due entirely to the American *operators*. It was *discovered* by them, and *adopted* by them."

This account of the origin of the *sounder* is given in his work published in 1860, more than twenty years after those very sounds had been specified and claimed in my Letters Patent as a part of my invention, and twelve years after the claim had been adjudged to be valid by the decision of a Circuit Court, and six years after this decision had been affirmed on appeal by the Supreme Co[illegible].

What is this Sounder?

The idea of making and preserving an automatic record of despatches transmitted by electricity, and particularly the device of Alphabetic characters or a Code of signs, adapted to such record when brought to public notice, was not at once perceived to be of unusual importance. The peculiar properties of the *new code* of signs began slowly to be developed and appreciated.

It was, however, soon perceived to be not merely the reduction of the alphabet to the simplest visible elements, to wit: *broken parts of a line*, but it was also perceived that these signs could readily be recognised by *any and each of the senses*, and therefore that they possessed an element of universality which commended them for other uses. They are not limited to telegraphic recording. Unlike any other code, each letter as well as numeral in the act of writing it, has its own peculiar *sound*. It may not be amiss, therefore, to give the cause of this early recognised peculiarity.

Each *letter*, as well as each *numeral*, when written is distinguished by the *eye* from all the others by its own peculiar combination of the *shorter* or *longer lines* and spaces.

But, in the act of writing, each sign for a letter or numeral indicates also to the *ear* the same difference of combination.

In writing a *dot*, or *short line*, the lever makes *two sounds:* the *first* as it strikes against the stop which limits its motion in one direction, and the *second* as it strikes in retreating against the stop which limits its motion in the other direction.

In writing a *long line* there are in like manner *two sounds* produced. The distinguishing difference between the two sounds for a short line, and the two sounds for a long line is, therefore, indicated, not by the *number* of sounds, but by the *interval* which occurs between the two sounds in each case. In writing a *dot* or *short line*, this *interval* is very *short*, while the interval between the two sounds in writing a *long line* is comparatively *long*.

Slight as this difference may seem it is sufficiently distinct to be recognised by the ear, and with great accuracy. This peculiar property of my telegraphic code was early noticed in the operation of the first instrument of 1835. Prof. Gale, in his examination before the Courts, thus alludes to his acquaintance with the fact of *reading by sound* as early as the spring of 1837. He says:*

"The fact that the said Morse's register indicates the message sent, both by *sounds* and permanent marking, was early noticed by all familiar with the invention, and was the subject of conversation and *experiment* between the said Morse and myself in the spring of 1837. So much knowledge was in fact conveyed by the *sounds* of the instruments, by those familiar with it, that not only messages could be read by the *sounds* of the instrument, but *the peculiarity of the person working the instrument at a distant station could readily be recognised from his manner of touching the key.*"

It is true the importance of receiving despatches by the ear was not at that time appreciated to the *extent* that it is at the

* Comp. Evid., French v. Rogers, p. 434.

present day, but it was sufficiently appreciated in the very outset to cause these *sounds* to be secured in the Letters Patent, lest this peculiarity, unless specified and claimed and thus secured, should be appropriated by others as their discovery, to the injury of my vested rights.

The language of my first Patent, applied for in 1838, has, therefore, these words, "*I specially claim* as my invention the use of the motive power of magnetism as means of operating machinery which may be used to *imprint* signals upon paper or other suitable material, or TO PRODUCE SOUNDS in any desired manner for the purpose of telegraphic communication at any distances."

The very first paragraph of my original Letters Patent designates my invention as "A new application and effect of electro-magnetism in producing SOUNDS and *signs*, or EITHER.

In the 8th claim of the Letters Patent, I claim the combination for "transmitting intelligence by *signs* and SOUNDS, or EITHER.

This claim was violated by certain parties. They were sued in consequence. The case was tried in a Circuit Court of the United States; the Court in pronouncing judgment, after declaring other acts of the infringers to be "a palpable violation of the injunction" which had been issued against them, says "that the operations of the same telegraph in receiving intelligence—IN AND BY SOUNDS *made by the same action of the telegraph which, in its regular operation, would have made the record in the telegraphic characters indented on the paper*—WAS A MERE EVASION *of the injunction, and substantially a violation thereof*, AND OF THE VESTED RIGHTS OF THE COMPLAINANTS." This decision was confirmed by the Supreme Court of the United States. This decision of the Circuit Court was made on October 24th, 1848; some twelve years before Mr. Prescott publishes as a fact, in his history of the telegraph, that "these *sounds* were an afterthought," the production of no one, but a sort of accident, discovered by some unknown operators," etc.

There are many other errors of a most serious character committed by this telegraphic historian, which, on revision for another edition of his work, I hope he will deem it advisable, for his own reputation's sake, to expunge and correct. The one I have exposed and confuted was too glaring to pass unnoticed. His quotations also from the Jackson answers in Court, I am reluctantly compelled to say, evince great credulity, ill concealed prejudice and dogmatism, and certainly very little discrimination.

On the principle laid down in my Introduction, that *matter of fact* is better than *misstatement*, even in the narrative of events of comparative trifling importance, I must correct the dates of two events to which some prominence has been given by Dr. Wynter, in his *Curiosities of Civilisation*, as well as by other writers, to wit:

The *first application* of the electric means of communication to *police purposes;* and

The *first game of chess* played by telegraph.

Dr. Wynter gives in detail the first application in England to *police purposes* in the detection and arrest of certain thieves and pickpockets at Slough, on Eton montem day, and the date, August 28th, 1844.

The *first* application of the telegraph in the United States for *police purposes* occurred between May 6th and 9th, 1844. The riots in Philadelphia, in which lives were lost and a church burned, occurred between those dates, and the telegraph between Baltimore and Washington was employed by the Philadelphia police, through the mayor of the city, imploring the aid of the United States troops, from the Federal government, to suppress the riots. The correspondence with the mayor and the decision of the government were conveyed by telegraph. This is believed to be the *first* use of the telegraph for *police purposes*, and antedates the English use for similar purposes by several months.

The *first game of chess* played by telegraph was also in the States, between the Washington and Baltimore Chess Clubs, previous to November 20, 1844, since the Baltimore *Patriot*, of that date, contains the full details, which are also preserved in Vail's work, at page 103.

The *first game* in England was in April, 1845, more than four months afterwards. The order of these events, therefore, stands thus:

The *first use for police purposes*,	United States,	May 6, 1844.
" " "	England,	Aug. 28, 1844.
The *first game of chess*,	United States,	Nov. 20, 1844.
" "	England,	April, 1845.

It must not be inferred that because a few of the errors of events, dates, and statements in the history of the telegraph have thus been exposed and confuted, that there are no others which might be in like manner exposed and confuted. There is scarcely a work which I take up, professing to give an history of the telegraph, that I do not find errors of more or less

importance, and they abound more especially in English publications.

The French and German writers are, as a class, far more reliable, have much less of narrow national or professional prejudice, and more enlarged and juster views of this comprehensive subject; but, even in the best of these, there are mistakes which need correction, but which, from the fairmindedness of the authors, as indicated by the general tone of their remarks, require only to be brought to the notice of their authors to have them promptly rectified.

When I speak of English publications, I include American as well as British writers. American writers on the telegraph may escape the charge, indeed, of national prejudice when they disparage my claims, but the prejudice of local interests, and the bias of opposition from rival companies, are quite as strong and wrong as any foreign prejudice, from whatever cause, can possibly be.

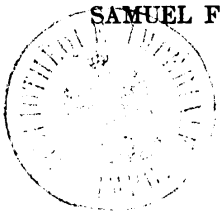
I am aware that there are still some flagrant instances of the grossest misrepresentation, and these, too, in high quarters, even before learned bodies, which I have, as yet, left unnoticed, not because they are not capable of complete exposure, but from very weariness.

I cannot but think that the corrections already made of *some* of the principal and fundamental errors may render it unnecessary to notice these, and others of less importance, most of them their natural offspring.

I may add, in conclusion, that this work has been necessarily interrupted by my recent appointment as a United States Commissioner to the Exposition Universelle, and, in that capacity, assigned to the duty of reporting upon the telegraph apparatus, a duty of a somewhat delicate character, considering my position. This duty, however, I shall endeavor impartially to perform, and my report, which requires time and careful study, will contain many incidental corrections more in detail, and, of necessity, some repetitions of facts already given in this work.

SAMUEL F. B. MORSE.

Paris, June, 1867.



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